PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IV. 1859–60. No. 51.

Monday, 19th December 1859.

DAVID MILNE HOME, Esq., in the Chair.

The following Communications were read:—

1. Note on some Numerical Relations between the Specific Gravities of the Diamond, Graphite, and Charcoal Forms of Carbon and its Atomic Weight. By Dr Lyon Playfair, C.B., F.R.S.

Recent researches have shown that there is an intimate relation between the specific gravities and atomic weights or equivalents of solid and liquid bodies. This relation is not so simple as that which prevails in regard to the volumes and combining numbers of gaseous bodies, and yet it is sufficiently marked to indicate many important chemical analogies. The formula for eliciting these relations is—

$$\frac{E}{d} = V,$$

in which E is the equivalent, d the specific gravities, and V the atomic volume.

It is to be borne in mind, that the unities or starting-points for specific gravities and for atomic weights are essentially distinct. In the first case, the weights of the bodies are compared with the weight of an equal bulk of water; in the second instance, the combining numbers refer to a unit weight of hydrogen. Nevertheless, the relations observed between the specific gravities and the atomic weights are well marked in bodies of a like character.

It has always been considered interesting to examine these relations in regard to Carbon, which has three well-characterised allotropic forms. The atomic volumes obtained by the above formula show no satisfactory relations between the numbers obtained for each of the states in which the element presents itself.

Before we examine them in another way, it is desirable to obtain a mean specific gravity for the Diamond, Graphite, and Charcoal, as the recorded results of experiment show a considerable variation.

1. *Diamond.*

The specific gravity of this gem is generally stated in elementary works to range from 3·5 to 3·55; but these numbers do not represent the mean of recorded experiments, as will be seen by the following table:—

Diamond in Hunterian Museum, Glasgow,	3·53	Thomson.[1]
Specific gravity, as stated by Mohs, .	3·52	Mohs.[2]
Brazilian diamond, . . .	3·44	Brisson.[3]
Another variety of the same, . .	3·52	
Mean specific gravity of a "beautiful collection of diamonds," . . .	3·48	Lowry.[4]
"Star of the south," . . .	3·53	Dufrenoy & Halphin.[5]
Borneo diamond, . . .	3·49	Grailich.[6]
Do. do., compact, . .	3·41	Rivot.[7]
Do. do., do., . .	3·25	

---

[1] Thomson's Mineralogy, vol. i. p. 46.

[2] Mohs' Mineralogy, vol. ii. p. 306.

[3] Brisson, as quoted by Böttger, Specifiche Geiwicht., p. 32.

[4] Lowry, as quoted by Thomson's Mineralogy, vol. i. p. 46.

[5] Dufrenoy, Compte Rendu, vol. xl. p. 3.

[6] Grailich Bull. Geol. [2], vol. xiii. p. 542.

[7] Rivot, Ann. des Mines, vol. xiv. p. 423.

Diamond used in Jacquelain's experiments,	3·33	Jacquelain.[1]
Specific gravity, as given by Henry, .	3·55	Henry.[2]
Well-crystallized Brazilian diamond, weighing 0·5761 gramme in the Edinburgh Museum, . . .	3·48	Playfair.[3]
Mean sp. gr.,	3·461	

If we reject the second Borneo diamond of Rivot, which has too low a specific gravity, we have a mean sp. gr. of 3·48, which is the same number as that found by Wilson Lowry for the mean specific gravity of "his beautiful collection of crystallized diamonds" (Thomson's Mineralogy, vol. i. p. 46).

It is to be expected that the experimental determination of the specific gravity of diamonds should be rather above than below the truth; for we are aware that they all leave a minute quantity of ash on burning, and that this ash, according to Petzhold, contains silica and iron.

2. *Graphite.*

This variety of carbon is often impure, being not unfrequently contaminated with upwards of five per cent. of earthy impurities. Recorded specific gravities upon such impure specimens are of no value for the mean result as regards pure graphite. The following determinations are all those which I can find upon specimens which have been chemically examined to establish their purity :—

Natural graphite, . .	2·27	Regnault.[4]
Do. . .	2·25	Schrader.[5]
Do. . .	2·32	
Graphite of iron furnaces, .	2·33	Karsten.[6]
Natural graphite, in fine crystalline plates,	2·14	Breikhaupt.[6]
Do. do., another variety,	2·22	
Do. do., do.,	2·23	Kengott.[7]

---

[1] Jacquelain, Ann. de Ch. et Thys, [2], vol. xx. p. 459.

[2] Henry's Mineralogy, vol. iv. p. 19.

[3] Experiment made for this paper.

[4] Regnault, Ann. de Ch. et Thys., vol. lvi. p. 37.

[5] Schrader, Annals of Philosophy, vol. i. p. 299.

[6] As quoted in Böttger's Specifiche Gewicht.

[7] Kengott, Wien Akad. vol. xiii. p. 469.

Natural graphite, . . .	2·50	Pelouze and Fremy.[1]
Gas carbon graphite, . .	2·35	Graham.
Mean sp. gr., .	2·29	

It would have been interesting to have added to this list a determination of the specific gravity of Brodies' purified Ceylon graphite ; but its minute division causes the air to adhere to it so tenaciously, that I have failed in getting any correct determinations of its density.

3. *Charcoal.*

There are comparatively few determinations of the specific gravity of pure charcoal. It is in fact not so easy to obtain this substance. A specimen of charcoal from pure sugar, repeatedly calcined, and treated with chlorine to remove the last traces of hydrogen, and again calcined, gave me the sp. gr. 1·80 ; but bubbles of air still adhered to it, although it was kept for several hours under a good air-pump. The following determinations are those recorded :—

Pure lamp-black, . .	1·78	Baudrimont.[2]
Fibrous gas coke, . .	1·76	Colquhoun.[3]
Compact gas carbon, .	2·08	Baudrimont.[4]
Powdered coke (mean), .	1·80	Regnault.[5]
Charcoal from alcohol, .	2·10	Scholtz.[6]
Charcoal from sugar, .	1·80	Playfair.[7]
Pure charcoal, without pores,	1·84	Griffith.[6]
Mean sp. gr.,	1·88	

4. From the preceding data we take the mean specific gravity of the three varieties of carbon to be as follows :—

---

[1] Traite de Chimie, vol. v. p. 518.
[2] Baudrimont, Traite de Chimie, vol. i. p. 511.
[3] Colquhoun's Annals of Philosophy [2], vol. xii. p. 1.
[4] Baudrimont, Traite de Chimie, vol. i. p. 514.
[5] Regnault, Traite de Chimie, vol. i. p. 369.
[6] Böttger Specifiche Gewicht.
[7] Experiment recorded above.

	Mean Sp. Gr.
Diamond,	3·48 or 3·461.
Graphite,	2·29
Charcoal,	1·88

5. We have now to consider whether these numbers stand in any simple relation to their atomic weight. The formula

$$\frac{E}{d}=V$$

gives the following atomic volumes, taking Ȼ = 12.

	Atomic Volumes.
Diamond,	3·44.
Graphite,	5·24.
Charcoal,	6·38.

These numbers do not bear to each other any simple relation.

6. If we now take the atomic weight of carbon (Ȼ = 12), and then extract from it its square, cube, and fourth roots, numbers are obtained which bear a striking approximation to the mean specific gravity of the three forms of carbon:—

	Roots.	Sp. Gr.
1	$\sqrt[2]{12} = 3{\cdot}464$	Diamond, 3·48 or 3·46.
2	$\sqrt[3]{12} = 2{\cdot}289$	Graphite, 2·29.
3	$\sqrt[4]{12} = 1{\cdot}865$	Charcoal, 1·88.

In other words, if we raise the specific gravity of diamond to its second power, that of graphite to its third power, and that of charcoal to its fourth power, we obtain numbers closely approaching in each case to 12, the atomic weight of carbon.

Diamond,	$3{\cdot}48^2=12{\cdot}11$.
Graphite,	$2{\cdot}29^3=12{\cdot}00$.
Charcoal,	$1{\cdot}88^4=12{\cdot}49$.

These approximations are remarkable, and the relations of the numbers are natural and simple. The differences between the mean experimental numbers and the corresponding roots of the atomic weight of carbon are not so great as the differences observed in the specific gravities of the same form of carbon.

7. It may be useful to condense into the form of a table the previous observations :—

Forms of Carbon.	Experiment.		Calculations.	
	Sp. Gr.	Powers.	~~C~~	Roots.
Diamond,......	3·46 or	$3{\cdot}48^2 = 12{\cdot}11$	$\sqrt[2]{12}$	$= 3{\cdot}464$
Graphite,......		$2{\cdot}29^3 = 12{\cdot}00$	$\sqrt[3]{12}$	$= 2{\cdot}289$
Charcoal, ...		$1{\cdot}88^4 = 12{\cdot}49$	$\sqrt[4]{12}$	$= 1{\cdot}865$

7

These relations appear to be so simple, that it is scarcely possible to conceive that they may not have been described before; but I have been unable to find such descriptions. The nearest approach to it which I know, is the fact that Mr Hawksley, the engineer, stated to me, many years since, that he had brought under the attention of the late Mr Cooper the relation which seemed to subsist between the specific gravities of silver and gold and their atomic weights, this being approximatively the square root of their atomic weights, or of multiples of these numbers. But I cannot find any record either of Mr Hawksley's or Mr Cooper's views on the subject.

8. We know two other bodies besides carbon which possess diamond, graphite, and amorphous forms—viz., Silicon and Boron. If the same relation were observed between the specific gravities and atomic weights of these bodies, it would go far to establish as a law what, in an isolated case, might be due to a remarkable combination of chances. Unfortunately, we know only the specific gravity of the diamond forms of these elements :—

Silicon diamond,	. .	2·49	Deville.
Do.,	mean on six specimens,	2·48	Playfair.

In quoting these results some explanation is necessary. In the original memoirs of Deville, it is left uncertain whether he examined the specific gravity of diamond or graphite silicon; and manuals of chemistry give it as the result due to the latter form; but from its coincidence with my own experiments on diamond silicon, it must unquestionably refer to that variety. Among the six specimens examined by myself, one preparation was in peculiarly fine crystals, and gave the sp. gr. 2·46; two out of the six specimens were prepared by Dr

Matthiesien, and gave a mean sp. gr. of 2·47. The remainder were inferior samples, and probably contained zinc and other impurities.

Professor Miller has kindly examined for me the specific gravity of a good specimen of graphite silicon in his possession (not analyzed), and found it to be 2·337.

Deville gives as the specific gravity of boron diamond 2·68.

The crystalline form of the boron diamond is the same as that of the carbon diamond, and similar relations seem to exist between the specific gravity and atomic weights. The atomic weight of boron is 7·2, viewing its oxide as corresponding to carbonic acid in composition.

$$\sqrt[2]{7\cdot2}=2\cdot683. \qquad \text{sp. gr.}=2\cdot68.$$

But the same relation would not appear to hold for silicon, which does not affect the like tendency to crystallize in the same forms as carbon and boron, although the relations between the numbers in its case also are in the same direction, and not devoid of simplicity.

The atomic weight of silicon is 14·2.

$\mathrm{Si}\ \sqrt[3]{14\cdot2}=2\cdot42$ sp. gr. of diamond silicon, $=2\cdot46$ to $2\cdot48$.
$\mathrm{Si}\ \sqrt[4]{28\cdot4}=2\cdot30$ sp. gr. of graphite silicon, $=2\cdot33$.

The differences exhibited in this case from the similar forms of Carbon and Boron are not sufficiently marked to throw doubt upon the relations as being due to some unexplained law. As an arithmetical probability, indeed, the discordance lessens the value of the testimony in the previous cases. But our chemical knowledge of the manner in which Silicon doubles and quadruples itself in the silicates, to unite with the same quantity of base, gives support to the idea that its atomic weights may be different in the various forms of the separate element.

When we consider how much we multiply the errors of experiment in raising the observed specific gravities to the second, third, and fourth powers, it remains scarcely possible that the simple relations between them and the atomic weights, in the cases which I have pointed out, can be due to chance. I have purposely avoided any speculation as to the bearing which these relations may have on the molecular arrangement of the particles of the elements in their various forms, as I desire, in the first place, to submit the testimony on which the relations themselves are founded to the consideration of chemists.

But it may be fairly asked, whether any similar relations exist between the specific gravities and atomic weights of the remaining solid or liquid non-metallic elements. I take the following mean specific gravity for bromine, iodine, sulphur, and selenium, and omit the only two remaining elements—phosphorus and tellurium—from the list, because they do not appear to yield relations at all analogous to those under consideration :—

Bromine, sp. gr., . . .	2·966	Balard.
	2·980	Lowig.
	2·990	„
Mean, . .	2·979	

Iodine.—There is only one recorded specific gravity of this element—viz., that by Gay Lussac. I have estimated the specific gravity of two fine specimens in my laboratory, and take the mean of these results :—

	4·948	Gay Lussac.
	5·030	Playfair.
Mean, . .	4·989	
Sulphur, sp. gr., ...		=2·0
Selenium, . .	4·30	Berzelius.
	4·32	„
	4·31	„
Mean, . .	4·31	

Tabulating these results, and bringing into comparison with them the roots of the atomic weights, we have the following striking accordances :—

	Sp. gr.	Equivt.	Roots.
Boron, . .	2·68	$\sqrt[2]{7{\cdot}2}$	= 2·68
Silicon . .	2·46	$\sqrt[3]{14{\cdot}2}$	= 2·42
Bromine, . .	2·98	$\sqrt[4]{80{\cdot}0}$	= 2·99
Iodine, . .	4·99	$\sqrt[3]{127{\cdot}0}$	= 5·02
Sulphur, . .	2·00	$\sqrt[4]{16{\cdot}0}$	= 2·00
Selenium, . .	4·31	Se$\sqrt[3]{80{\cdot}0}$	= 4·31

2. Some Miscellaneous Observations on the Tadpole, and on the Albumen of the Newly-laid Egg. By John Davy, M.D., F.R.S. Lond. and Edin.

On the Tadpole of the Frog.—The author *first* notices the jelly—the peculiar enveloping matter of the ova—describing its properties and uses,—the latter, according to him, for the double purpose of defending the eggs before being hatched, and affording the tadpoles food after their hatching. He considers the substance of the jelly a variety of albumen.

Secondly, He notices the ova, which he describes as resembling, in their properties and composition, those of fishes.

Thirdly, He enters into some details respecting the growth of the tadpole and its metamorphosis, specially dwelling on the fact, that whilst in the change to the advanced form there is a diminution of volume, there is an increase of solid matter—the young frog having a bony skeleton, which the tadpole is destitute of.

Fourthly, Experiments are given showing the effect of different degrees of temperature on the tadpole, and that of salt water of different degrees of saltness. From the last it would appear that even brackish water is fatal to them, thus limiting the habitat of the species; from the former, that a temperature exceeding 98° or 100° has the same effect; whilst one a little lower seems to promote a torpid state, such as the alligator is said to acquire during the season of tropical heat and drought.

On the Albumen of the Newly-laid Egg of the Common Fowl.—From the experiments described, it would appear that the albumen of the newly-laid egg differs from that of the egg kept for some time: 1st, In becoming milky at about 150° Fahr., owing to the formation of innumerable granules, so small as barely to be seen with the microscope,—a quality which may be preserved for a considerable time by the exclusion of oxygen, effected by lubricating the shell with oil or butter; 2dly, In forming, at a higher temperature, a softer coagulum.

3. On Acupressure, a New Method of Arresting Hæmorrhage. By Professor Simpson.

Professor Simpson made a communication on acupressure, as a new mode of arresting surgical hæmorrhage. After describing the various methods of stanching hæmorrhage in surgical wounds and operations which the Greek, Roman, Arabic, and Mediæval surgeons employed, he gave a short history of the introduction of the ligature of arteries, and spoke of it as—with the occasional exception of torsion for the smallest arteries—the hæmostatic means almost universally employed in chirurgical practice at the present day. But he thought that surgery must advance forward a step farther than the ligature of arteries, particularly if surgeons expected—as seemed to be their unanimous desire—to close their operative wounds by the immediate union or primary adhesion of their sides or walls.

All the march of modern surgery has been in the direction of attempting to increase the chances of the union of surgical wounds by the first intention, by diminishing more and more the irritation derived from the presence and action of the ligatures supposed to be inevitably required for the arrestment of the hæmorrhage. By the new hæmostatic process of acupressure, Dr Simpson hopes to overcome in a great degree all those difficulties, as by it he expected to arrest the hæmorrhage attendant upon surgical wounds *without leaving permanently any foreign body whatever* in the wound itself. It was an attempt to bring bleeding wounds, in common surgery, to the condition of wounds in *plastic surgery*, where no arterial ligatures were used, and where union by the first intention was in consequence the rule, and not the exception to it.

Dr Simpson stated that he had tested, with perfect success, the effects of acupressure as a means of effectually closing arteries and stanching hæmorrhage, first upon the lower animals, and lately in two or three operations on the human subject. The instruments which he proposed should be used for the purpose were very sharp-pointed slender needles or pins of passive or non-oxidizable iron, headed with wax or glass, and in other respects also like the hare-lip needles commonly used by surgeons at the present day, but longer when circumstances required. They might be coated with silver or zinc on the surface, if such protection were deemed requisite.

That needles used for the purpose of acupressure, and passed freely through the walls and flaps of wounds, will not be attended by any great degree of disturbance or irritation, is rendered in the highest degree probable by all that we know of the tolerance of living animal tissues to the contact of metallic bodies. Long ago John Hunter pointed out that small-shot, needles, pins, &c., when passed into and imbedded in the living body, seldom or never produced any inflammatory action, or none at least beyond the stage of adhesive inflammation, even when lodged for years. Some time ago, when the subject of acupuncture specially attracted the attention of medical men, Cloquet, Pelletan, Pouillet, and others, showed that the passage and retention of long acupuncture needles was attended with little or no irritation in the implicated living tissues. The reviewer of their works and experiments in the *Edinburgh Medical Journal* for 1827 observes,—" It is a *remarkable* circumstance that the acupuncture needles never cause inflammation in their neighbourhood. If they are rudely handled or ruffled by the clothes of the patient, they may produce a little irritation; but if they are properly secured and protected, they may be left in the body for an *indefinite* length of time without causing any of the effects which usually arise on account of the presence of foreign bodies. In one of M. Cloquet's patients, they were left in the temples for eighteen days; and in cases in which needles have been swallowed, they have remained without causing inflammation for a much longer period. It appears probable, from the facts collected on the subject, that metallic bodies of every kind may remain imbedded in the animal tissues without being productive of injury." (Page 197.) All the late observations and experiments upon metallic sutures are confirmatory of the same great pathological law, of the tolerance of living tissues for the contact of metallic bodies imbedded within their substance. In the operation for hare-lip, where the whole success or failure of the operation depends on the establishment or not of union by the first intention, surgeons use needles to keep the lips of the wound approximated, often compressing these needles strongly with their figure-of-eight ligatures, and find this measure the most successful means which they can adopt for accomplishing primary adhesion.

The acupressure of arteries, when compared with the ligature of them, appears, as a means of arresting hæmorrhage, to present various important advantages :—

1st, Acupressure will be found more easy, simple, and expeditious in its application than the ligature.

2d, The needles in acupressure can scarcely be considered as foreign irritating bodies in the wound, and may always be entirely removed in two or three days, or as soon as the artery is considered closed; whilst the ligatures are truly foreign irritating bodies, and cannot be removed till they have ulcerated through the tied vessels.

3d, The ligature inevitably produces ulceration, suppuration, and gangrene at each arterial point at which it is applied; whilst the closure of arterial tubes by acupressure is not attended by any such severe and morbid consequences.

4th, The chances, therefore, of the union of wounds by the first intention should be much greater under the arrestment of surgical hæmorrhage by acupressure than by the ligature.

5th, Phlebitis, Pyæmia, &c., or, in other words, traumatic or surgical fever, seem not unfrequently to be excited by the unhealthy local suppurations and limited sloughings which are liable to be set up in wounds by the presence and irritation of the ligatures.

6th, Such dangerous and fatal complications are less likely to be excited by the employment of acupressure, seeing the presence of a metallic needle has no such tendency to create local suppurations and sloughs in the wound, such as occur in the seats of arterial ligatures.

And 7th, Hence, under the use of acupressure, we are entitled to expect both, *first*, that surgical wounds will heal more kindly and close more speedily; and, *secondly*, that surgical operations and injuries will be less frequently attended than at present by the disastrous effects and perils of surgical fever.

The following Donations to the Library were announced:—

Transactions of the Botanical Society of Edinburgh. Vol. VI., Part II. 8vo.—*From the Society.*

Monthly Notices of Astronomical Society. Vol. XX., No. 1. 8vo. —*From the Society.*

Proceedings of the Linnean Society. Vol. IV., No. 15. 8vo.— *From the Society.*

Canadian Journal. September and November 1859. 8vo.—*From the Publishers.*

Journal of Statistical Society. December 1859. 8vo.—*From the Society.*

Transactions of the Royal Medical and Chirurgical Society of London, 1859. 8vo.—*From the Society.*

Madras Journal of Literature and Science. April to September 1858. 8vo.—*From the Madras Literary Society.*

Transactions of Bombay Geographical Society. Vol. XIV. 8vo. —*From the Society.*

Aanteekeningen van het Verhandelde in de Sectie-Vergaderingen van het Provinciaal Utrechtsche Genootschap van Kunsten en Wetenschappen. 1855–1859. Utrecht. 8vo.—*From the Society.*

Verslag van het Verhandelde in de Algemeene Vergadering van het Provinciaal Utrechtsche Genootschap van Kunsten en Wetenschappen. 1856–59. 8vo.—*From the Society.*

Annales de l'Observatoire physique centrale de Russie. 1856. 4to.—*From the Observatory.*

Compte Rendu Annuel. 1857. 4to.—*From the Academy of Sciences.*

Report on Canadian Graptolites. By James Hall, Esq. Montreal, 1858. 8vo.—*From the Author.*

Tuesday, 3d *January* 1860.

PROFESSOR MORE in the Chair.

The following Communications were read:—

1. Some Miscellaneous Observations on the Growth of Birds, their Specific Gravity, and on the Stomach of Fishes in Relation to Digestion. By John Davy, M.D., F.R.S. Lond. and Edin.

On the Growth of Birds.—The author's observations on this subject are chiefly confined to the martin, the common fowl, the turkey, and goose. They all tend to show a rapid growth, varying in degree according to the habits of the species. The young martin was found on leaving its nest heavier than the parent bird; and this the growth, as to time, of about twenty days, reckoning from the hatching of the egg. A turkey poult, the day it quitted the egg, weighed one ounce and three quarters; in five months it had increased to ten pounds. A gosling, in thirty-four days, had increased in weight from six ounces to six pounds. A chick of the Dorking breed, in three months, had increased from an ounce and a half to three pounds. This rapidity of growth—a rapidity in the instance of nestlings fed by the parent birds essential to their existence—is referred to two principal causes, an active digestion and abundance of food, other circumstances aiding. The relation of the size of the egg of different species to that of the parent birds is suggested as an interesting subject for inquiry; and the conjecture is offered, illustrated by some examples, that the circumstances chiefly influencing that relation may be the manner in which the young birds are fed, whether by the old birds, or unaided by them, and the proportional abundance of food.

On the Specific Gravity of Birds.—The birds submitted to trial were the martin, the water-ouzel, snipe, wood-owl, merlin-hawk, and wren. These, deprived of their feathers, were found to be of nearly the same specific gravity as the water in which they were weighed. With the feathers on, in common with birds generally, they all floated in water, buoyed up by the lightness of their feathers, —these so light owing to the air they contain. As the air becomes disengaged on forced submersion, so the specific gravity of the bird increases. A wren, the specific gravity of which when first sunk in water was 0·890, after a continuance under water of twelve hours, had increased to 0·960. The specific gravity of the water-ouzel with its feathers on was found to be about 0·724; that of the merlin-hawk about 0·570. It is pointed out, in conclusion, how little the specific gravity of the bird is concerned as regards aptitude for aerial locomotion, and how it is subordinate to various circumstances, such as lightness from included air and a high temperature, and a great impulsive power of wing.

On the Stomach of Fish in relation to Digestion.—In the instances of the salmon and sea-trout, and indeed of all the fish tried—these including the common trout, charr, grayling, haddock, and dogfish—the stomach, when empty, was found by test-papers to be commonly neutral, and on the contrary acid when any food was present; leading to the conclusion that the gastric juice is secreted only when the organ is stimulated and that fluid is required. From observations on fish after death, kept for a certain time, sufficiently long for the fluids to act on the containing solids, denoted by softening and rupture, the inference is come to that the fluid of the appendices pyloricæ is most active in producing the post-mortem effects; and that the migratory species of the salmonidæ, the salmon and sea-trout, taking little food in fresh water, exhibit least this effect, and are least subject, from the emptiness of their primæ viæ, to putrefaction.

2. Notice of certain Remarkable Laws connected with the Oscillations of Flexible Pendulums: With Illustrative Experiments. By Edward Sang, Esq.

In a previous paper there had been given a general method of investigating the motions of elastic systems, when the redressing tendencies are proportional to the extents of the disturbance. The present paper contains the results of the application of this method to the oscillations of flexible pendulums, composed of weights attached by their centres of gravity to a thread. These results, though strictly applicable only to oscillations of infinitely small extent, may yet be held as indicating the general characters of oscillations of moderate dimensions.

The exceedingly complex motions of such a pendulum may always be resolved into as many simple oscillations as there are bodies in the system; a simple oscillation being such that, if it subsisted alone, all the moving points would pass through their mean positions at the same instant, and would also all reach the extreme limits of their motion at once.

The periodic times and the configurations of those simple motions can be computed by help of an equation rising to the degree indicated by the number of the bodies; and we obtain this very singular result, that "*Whatever may be the details of the system, the sum of the squares of these periodic times is equal to the square of the periodic time of a simple pendulum having the entire length of the flexible one;*" or, in other words, that "*If simple pendulums be constructed vibrating in accordance with the simple oscillations of such a flexible series, the sum of their lengths is equal to the whole length of the flexible line.*"

In order to cause any given pendulum to perform one of its simple oscillations, we would need to give to each one of its component parts a properly regulated impulse: thus we might compute their extreme positions; and, having placed each body properly, let them all go at the same instant. Hence it is a matter of great difficulty to exhibit a simple oscillation when the system consists of even so few as three or four parts.

When all the bodies are of one weight and uniformly distributed along the chord, the configuration of a simple oscillation is given by the formula—

$$x_n = x_A \left\{ 1 - \frac{n}{1}\left(\frac{l}{p}\right) + \frac{n}{1}\,\frac{n-1}{4}\left(\frac{l}{p}\right)^2 - \frac{n}{1}\,\frac{n-1}{4}\,\frac{n-2}{9}\left(\frac{l}{p}\right)^3 + \&c. \right\}$$

in which l is the length of one of the links, p that of a simple pendulum oscillating in the same time, x_A the ordinate of the lowest body A, and x_n that of a body situated n intervals above A.

When a flexible pendulum consists of two weights A and B, attached by their centres of gravity to two threads AB, BC of equal lengths, the ratio of the periodic times of its two simple oscillations may be exhibited by constructing a right-angled trigon PQX, such that the square of PQ may be proportional to the weight A, the square of QR to the weight B; this being done, the periodic time of the slower is to that of the quicker oscillation as QP + PR : QR.

The truth of this law was exhibited experimentally by making A nine and B sixteen ounces: an arrangement which gives the periodic times as 2 to 1; and also by making A sixteen and B nine, in which case the ratio is as 3 to 1.

When, by augmenting indefinitely the number of the weights, we pass from the discrete series to a continuous uniform flexible line, the previous equation takes the form

$$x = x_A \left\{ 1 - \frac{1}{1}\left(\frac{z}{p}\right) + \frac{1}{1}\,\frac{1}{4}\left(\frac{z}{p}\right)^2 - \frac{1}{1}\,\frac{1}{4}\,\frac{1}{9}\left(\frac{z}{p}\right)^3 + \&c. \right\}$$

in which p is the length of the corresponding simple pendulum, z the distance upwards from the lowest point A, and x_A the horizontal ordinate of A. These formula differ, the one from the well-known development of $\left(1 - \frac{l}{p}\right)^n$, the other from $e^{-\frac{z}{p}}$ in having for the denominators of the successive co-efficient the squares of the natural numbers instead of those numbers themselves.

From this latter equation it follows, that if a uniform flexible chain could be made to perform one of its simple oscillations, its configuration would always have the character of the curve so indicated. Starting from the lowest point in a direction which, if continued, would cross the vertical axis at the height p, this curve crosses and re-crosses that axis, each wave being longer and flatter than the preceding: the first crossing is at the height $p \times 1{\cdot}445\ 7965$; the second at $p \times 7{\cdot}617\ 8156$; and the third at $p \times 18{\cdot}721\ 7517$: hence the slowest oscillation of a uniform flexible chain of which l is the length accords with that of a single pendulum $l \times \cdot 691\,6603$ long: the second oscillation agrees with that of a simple pendulum having $l \times \cdot 131\,2712$ for its length, while the third oscillation corresponds to $l \times \cdot 053\,4138$.

The three oscillations of a chain were exhibited, and were shown to agree exactly with the computed times as well as with the figures which had been prepared.

In conclusion, it was remarked that, although these speculations be of interest chiefly to those whose attention is particularly turned to mechanical science, they are not devoid of attractions for the student of general physics, as they bear closely upon the received doctrines of acoustics, and also upon the undulatory theory of light. In these doctrines it is assumed, as the very foundation on which they are built, that the disturbance of an elastic medium engenders a series of waves which progress through that medium producing, in the case of air, the phenomenon of sound,—in the case of the supposed luminous ether, waves of light; yet a strict investigation shows that, when an elastic system is deranged at any one point, all the species of vibration of which the system is capable are at once called into existence, and that the isolation of one of the simple oscillations would require the simultaneous action of as many distinct and properly regulated impulses as there are particles in the system: in other words, it shows that the formation of the supposed aërial pulses, or of the imagined luminous waves, is physically impossible.

When we strike the string of a harp or of a clavichord, we do not cause it to perform its major vibration alone—on the contrary, we produce many of its partial motions also; and although the number of beats per second of its slowest oscillation be the principal argument of the theoretical musician, it by no means follows that those beats constitute more than an important element in what is truly the sound of the string.

The following Donations to the Society were received:—

Nautical Monographs. No. 1. 4to.—*From the Observatory, Washington.*

Silliman's American Journal. No. 84. Second Series. 1859. 8vo.—*From the Editors.*

Journal of Asiatic Society of Bengal. No. 3. 1859. 8vo.—*From the Society.*

Proceedings of the Academy of Natural Sciences of Philadelphia. 1859. 8vo.—*From the Academy.*

Extinct Vertebrata from the Judith River, and Great Lignite Formations of Nebraska. By Joseph Leidy, M.D. Philadelphia. 1859. 4to.—*From the Author.*

A Memoir on the Extinct Sloth Tribe of North America. By Joseph Leidy, M.D. 4to.—*From the Author.*

The Ancient Fauna of Nebraska. By Joseph Leidy, M.D. 4to.—*From the Author.*

Descriptions of some Remains of Fishes from the Carboniferous and Devonian Formations of the United States. By Joseph Leidy, M.D. Philadelphia. 1856. 4to.—*From the Author.*

Journal of the Academy of Natural Sciences of Philadelphia. 1859. 4to.—*From the Academy.*

Observations on the Genus Unio. By Isaac Lea, LL.D. Vol. VII., Part I. Philadelphia. 4to.—*From the Author.*

Lever's Year-Book and Railway and Mining Almanac. Manchester. 1860. 8vo.

Proceedings of Horticultural Society of London. No. 7. 1859. 8vo.—*From the Society.*

Monday, 16*th January* 1860.

LORD NEAVES, V.P., in the Chair.

The following Communications were read:—

1. Suggested Explanation of Messrs Carrington and Hodgson's recently observed Solar Phenomenon. By Professor C. Piazzi Smyth.

The Royal Society of Edinburgh having been the arena wherein Professor W. Thomson first described his calculations and admirable extensions of Mr Waterston's meteoro-dynamic theory of solar light and heat, I beg leave to call the attention of the same learned Society to an apparent instance of confirmation which that theory appears to me to have received, by a phenomenon of very unique character, recently observed in an independent and most satisfactory manner, by either of two able scientific men—viz., Mr Carrington of the Observatory, Red Hill, and Mr Hodgson at Highgate.

The respective observations of these gentlemen are to be found in the monthly notices of the Royal Astronomical Society for November 1859, and seem quite sufficient to prove, after making all due allowance for the different instrumental methods employed in either case, and the peculiar nature of the subject observed, that on September 1, at about 11 h. 18 m. A.M., Greenwich time, two small telescopic bodies of light, in close proximity, and elongated in the direction of their motion, suddenly burst into view on the surface of the sun, not very far from its central portion, than which they were very much brighter. They moved side by side in arcs nearly parallel with the plane of the ecliptic; first for a time increasing in brightness, and then again gradually fading away, so as to be quite lost in about five minutes after their first appearance. Though apparently on the surface of the sun, yet that appearance was considered to arise from optical projection only, as they did not alter the shape of a group of large black spots, which lay directly in their paths. They must, nevertheless, have been exceedingly close to the surface; and on that supposition, the paths which they described during their period of visibility must, from their angular extent, have measured about 35,000 miles, giving a mean rate of 117 miles per second.

The first remark that we may make on the facts of observation, save that nothing so momentary has ever been witnessed before by astronomers, is, that 117 miles per second constitutes a velocity so exceedingly great, that we can only look to the gravitation influences of the sun for its efficient producing cause. Nevertheless, we are at the same time bound to acknowledge, that the full rate of orbital motion, for a body nearly in contact with the surface of the sun, is rather over 276 miles per second; and the rate of falling to the sun from infinite space, considerably more. Evidently, then, something prevented these bodies of September 1 from moving at their full rate, and produced a retardation in their orbit equal to 159 miles a second, to take Professor W. Thomson's form of the gravitation theory as the more probable.

What that retarding something was, it is not so much to our purpose now to inquire, as long as we can show that it is not altogether a baseless supposition, to assume the existence of any extensive material belonging to the sun, outside his visible, luminous surface.

Now, this is fortunately easy of demonstration, for we have only to point to the red prominences, borders, and clouds, seen stretching to a distance of 60,000 miles sometimes from the edge of the solar orb during solar eclipses; or to those unmistakable evidences of a large amount of outer absorbent and obstructing atmosphere, indicated by the gradual yellowing and de-photographical properties of the sun's light, with every advance from the centre to the circumference of his disc. (See Teneriffe Report, "Philosophical Transactions" for 1858, p, 487.) Something material, therefore, and forming to all intents and purposes one body with the sun, does exist round about him; and capable, we cannot but allow, of affording impediment to the motion of any extraneous body, before it actually comes into contact with that intensely bright surface which is vulgarly held to constitute the outside of the sun.

Hence, when we find our solar meteors of September 1 moving at a rate slower by 159 miles a second than they should do according to the laws of gravitation, the simplest assumption that we can make is, that there has been a mechanical retardation to that amount. Let this be granted, and then it necessarily follows from the dynamical theory of heat, that precisely in accordance with the disappearance of motion will be the appearance of heat. And when we further take account of the previous high temperature to which the matter of the meteor (for the two, we are inclined to look on as fragments of one; unless Bielas' double comet is to be taken as an argument for constant double meteors also) was brought, during the long ages of its circulation around the sun, in an orbit whose mean distance decreased only secularly; and when we also consider the extraordinary warmth of the region where the observed rapid retardation took place, it will be understood that everything was extremely favourable to the heat produced by conversion of motion, appearing with so intense an expression, as to be accompanied by a most vivid display of light.

Even in the cold external atmosphere of the earth, and with a speed of only 19 miles in a second on the average to be converted, M. Joule has shown that meteors should become intensely white hot; and Professor Swan has described in the "Proceedings" of this Society, Vol. III., p. 220, a case of one bright enough to be conspicuously seen at noonday.

It is true that, according to Mr Waterston's form of the theory, and even the first expression of Professor W. Thomson's also, where the sun is fed by lumps of meteoric matter, there is some difficulty in explaining why the occurrence of luminous meteors in the sun is not frequently observed, if remarkably visible in one instance; while, according to the subsequent modification of the latter's view, the generality of meteors must be distilled away into impalpable clouds of finely-divided meteoric matter some time before they actually reach the sun; and neither gentleman had expected that an actual impact would ever be seen by mortal eye.

These objections, however, will be at once, to a great extent, relieved by the very fair assumption of as superior a mass to the September 1st meteor, over the generality of those which fall to the sun, in any and every manner, as men have already recorded of those which have actually fallen to, or have been seen very near, the earth; for while the majority (see the museums of Vienna, St Petersburg, and London) measure only a few inches, with occasional masses of 2 and 3 feet, there was one unusually well observed by many able spectators in Scotland, England, and France, on the 18th August 1783, which was estimated to be more than half a mile in length.

It may indeed be argued that, with this superior size of body, the decrease of the meteor's *radius-vector* in its orbit would have been slower than with smaller lumps revolving in the denser parts of the ethereal medium in the sun's immediate neighbourhood (see "Edinburgh Astronomical Observations," vol. xi. p. 266); and it would therefore have been exposed so much longer to the fiery heat of that region as to be distilled away, equally with the smaller ones, before actually touching the grosser parts of the solar atmosphere.

This argument, even carried to the extreme, does but remove the probability of the phenomenon of September 1st from being a meteor of the W. Thomson character, circulating around the sun, to one of Waterston's, falling to the sun from distant space; and while Professor W. Thomson has most satisfactorily demonstrated that, whatever may be the real character of the greater part of the solar light and heat, *some* of it must be due to meteoric action, so, while we freely concede to him that the majority of meteoric matter falling to the sun is of the planetary or satellite character, we are still entitled, according to his own reasoning on p. 67 of "Transactions of Royal Society of Edinburgh," vol. xxi. part i., as well as from some of the paths computed by M. Petit, of the Toulouse Observatory, to assume that some meteoric matter arrives from distant space, and falls nearly directly to the sun. In such case, the final rate of its motion being 390 miles per second, there would be small time for much more to be effected, than that that fused crust we so often find on terrestrial meteoric stones should be formed, prior to the commencement of rapid retardation, and before conversion of mechanical energy into heat; followed, doubtless, almost immediately by the breaking up of the matter from explosion and deflagration.

Let us inquire, then, how far the known meteoric mass of 18th August 1783 would suffice to produce the effects observed on September 1st, 1859, had it then fallen to the sun. Professor W. Thomson gives the amount of meteoric matter that would be required, according to Mr Waterston, to produce, by striking the sun, the average solar illumination, as 0·000060 lb. per square foot per second.

For the period of our phenomenon, or 5 minutes, this is ·018 lb. per square foot, and 501·811 lb. per square mile for the same time.

Now the meteor in question, said by Mr Cavalho to have had a diameter of 1070 yards, can hardly have contained less than 15,000,000,000 cubic feet; and if we take for its specific gravity a mean between what has been determined by many measures of earthy meteorites on one hand, and meteoric iron on the other (which comes extremely near the mean density of the earth), then the total weight must have been 5,250,000,000,000 lb. Whence it is evident that there was enough material in that one meteor, properly distributed, to keep a space 5,000,000 square miles of the sun's surface in a state of luminous ignition, twice as intense as that of the ordinary solar disc during all the time of observation; and therefore, by the transparency of flame, to have tripled the brightness of the parts passed over—a phenomenon which, from angular subtense, as well as intensity of light, would be abundantly visible to telescopic observation from our earth.

* To this we may further add another consideration, which would notably increase the quantity of light given out by the same meteor; for in the remarkable investigations which Professor W. Thomson has appended to his paper above quoted, relative to the

rotation and the age of the sun, he appears to have assumed, if I read him aright, that all the meteors in the solar neighbourhood are circulating in the same direction as the planets; and it is extremely probable that this is true for the greater number of them. Indeed, the publication in France, since this paper was placed in the hands of your Secretary, of the apparent discovery of a planet interior to Mercury, at considerably less than half his heliocentric distance, and revolving *direct*, might be taken as a remarkable proof of the correctness of the assumption. Nevertheless, we cannot close our eyes to the fact, that the members of the solar system which occasionally come visibly closest to the sun are not planets, but comets, and that, amongst these, retrograde motion is frequent; while of all comets, the one which has made the closest known approach, since the age of exact astronomy, was that of 1843, which was retrograde, and passed only 60,000 miles from the sun's surface. Hence it is by no means an unwarranted supposition for us to make, that an occasional meteor moves *retrograde* also; as, in fact, this one of September 1st appears to have done; and if it should, in such a course, encounter another moving *direct*, in the crowded solar vicinity—a casualty that must become more and more probable with every advance towards the sun—there would be double speeds of motion to be converted into heat; there would be much such a sudden blaze of light as the observers described; and then a rapid descent of both bodies to the sun's surface, through that remaining distance in which they might otherwise have long continued to circulate before their final absorption.

---

* This paragraph added on January 16, 1860.

On the whole, then, it appears exceedingly probable that the solar phenomenon of September 1st was a meteor falling to the sun, and giving out the heat of its mechanical energy in accordance with the laws of that dynamical theory of thermotics, first and chiefly in this country brought before the Royal Society of Edinburgh by Professors W. Thomson and Macquorn Rankine. In which case, there is another example added, to several that might be extracted from the history of science, showing that hardly has a true theory been published to the world, before a confirmatory phenomenon, previously quite unexpected, is almost providentially witnessed; and in no case by less prejudiced or more able observers than the gentlemen upon whom we depend in the present instance.

2. On the Fallacy of the Present Mode of Estimating the Mean Temperature in England. By James Stark, M.D.

In the Meteorological Tables for England, published at the public expense, viz., those of Greenwich Observatory, and those of the fifty-five meteorological stations in England, appended to the Registrar-General's Quarterly Reports, the mean temperature as given is not the true mean, but is an estimated mean, deduced from the united observations made with the self-registering and dry-bulb thermometers, the exact means of these instruments being first altered by certain tables, with the view of correcting them for diurnal and monthly range. The author objected to this mode of estimating the mean temperature, because the tables used for the purpose of correction were drawn up from insufficient data, and because the principle was bad of having observations made by one instrument, whose readings were liable to constant errors, made the basis for the correction of observations made by a more trustworthy instrument.

The tables for the correction of diurnal range used in England were drawn up from a five years' series of observations; but by a reference to the Scottish observations, it was attempted to be shown that a five years' period was far too short to elicit even a true mean annual temperature ; while, by a reference to the mean temperature of corresponding months during five consecutive years, it was found that such a period gave still more incorrect results for the months.

It was then demonstrated, by a reference to the Greenwich monthly and daily observations, that the readings of the dry-bulb thermometer, at every two-hourly period, failed to give the true mean temperature either of the day or of the month, as they always missed the period of the day when the maximum temperature was attained, which, as indicated by the self-registering thermometer, was from 1° to 5° higher than any of the readings taken at the two-hourly periods. The consequence was, that the mean temperature deduced from the dry-bulb two-hourly readings was always *below* the truth, and it was impossible from them to ascertain at what period of the day the highest temperature was attained, so that the chief elements for the calculation of the amount of diurnal range were wanting.

The data, however, which were used for the formation of tables of correction for the self-registering thermometric observations were shown to be even more faulty than those used for that of the dry-bulb; *firstly*, from the circumstance that the faulty dry-bulb mean was held to be the only true mean, and made the basis for the correction of the self-registering mean; and *secondly*, from the circumstance that the whole of the self-registering observations were made with Six's registering thermometer, an instrument now considered inaccurate. By a reference to the Makerstoun Observations for a period of three years, and to those of Scotland for the last two years, it was demonstrated that, *when the self-registering thermometers are of proper construction*, the strict mean of the maximum and minimum readings appear to require no correction whatever, but indicate the true mean temperature. Particular attention was directed to the Makerstoun Observations for 1844, when hourly readings were taken with the dry-bulb thermometer, their strict mean giving the mean annual temperature as 44°9, while the strict mean of Rutherford's self-registering thermometer gave the annual temperature as 45°0, than which no results could be closer.

In the present state of the science, the author contended that the only safe rule for the estimation of mean temperature was to take the strict mean of the self-registering thermometers, provided these were of proper construction; and he strongly denounced the practice followed in England, of withholding the strict means of the different series of dry and wet bulb readings, and only publishing deductions, which the facts brought forward in his paper proved must, in most instances, be erroneous.

3. Description of the Plant which produces the Ordeal Bean of Calabar. By Professor Balfour.

After noticing the various plants used in Africa as ordeal poisons, the author gave an account of the introduction of the Calabar Ordeal Bean into Scotland, by the Rev. W. Waddell, and mentioned its peculiar poisonous qualities, as determined by Dr Christison. To Dr Hewan, and the Rev. Zerub Baillie, who are connected with the United Presbyterian Mission in Old Calabar, he was indebted for some observations on actual cases of poisoning in Africa. The Rev.

W. C. Thomson, another missionary, was the first who procured flowering specimens of the plant. Some of these had been given to the author by Mr Baillie, and from them, along with the legume and seeds, the characters of the plant had been drawn up. The plant belongs to the natural order Leguminosæ, sub-order Papilionaceæ, and tribe Phaseoleæ, and appears to be a new genus to which the name of *Physostigma* (φυσάω, to inflate) has been given, from the peculiar inflated appearance of the stigma. To the species the name of *venenosum* has been given, in allusion to its poisonous qualities. The genus is nearly allied to Phaseolus, from which it differs in the stigma, and in the long, grooved hilum of the seed. In the last character it approaches Mucuna.

Physostigma venenosum is a large twining plant, with a thick stem, and pinnately-trifoliolate leaves. The inflorescence is nodoso-racemose, the flowers being curved, and of a pale pink colour, the stamens 10, diadelphous, the style bearded at its upper part, and the stigma covered with a remarkable crescentic ventricular sac. The legume is 7 inches long, of a brown colour, containing two or three dark-brown seeds, with a long, deep hilum.

The paper was illustrated by drawings, executed by Dr Greville.

The following Donations to the Library were announced :—

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences, Nos. 21–24. 4to.—*From the Academy.*

Transactions of the Pathological Society of London. Vol. X. 1859. 8vo.—*From the Society.*

Monthly Notice of Astronomical Society. Vol. XX., No. 2. 8vo.—*From the Society.*

Journal of the Geological Society of Dublin. Vol. VIII., Part 2. 1859. 8vo.—*From the Society.*

Instructions and Chart for Observations of Mars in right ascension at the opposition of 1860 for obtaining the measure of the Sun's distance. By G. B. Airy, Esq., Astronomer-Royal. 8vo.—*From the Royal Astronomical Society.*

Memoirs of the Geological Survey of India. Vol. II., Part 1. 1859. 8vo.—*From the Indian Government.*

Atti dell' I. R. Istituto Lombardo di Scienze, Lettere ed Arti. Vol. I., Parts 12–14.

Ofversigt af Kongl. Vetenskaps-Akadémiens Förhandlingar. 1858. 8vo.—*From the Royal Society of Sciences of Stockholm.*

Berättelse om Framstegen i Fysik under år 1853, af E. Edlund. 1859. 8vo.—*From the same.*

Berättelse om Framstegen i Insekternas, Myriapodernas och Arachnidernas Naturalhistoria för 1855 och 1856, af C. H. Boheman. 1859. 8vo.—*From the same.*

Kongliga Svenska Fregatten Eugenies resa omkring Jorden under befäl, af C. A. Virgin. Aren., 1851–1853. Häft 6. 1859. 4to.—*From the same.*

Kongliga Svenska Vetenskaps-Akadémiens Handlingar. Bd. II. Häft 1. 1857. 4to.—*From the same.*

The Assurance Magazine and Journal of the Institute of Actuaries. Jan. 1860. 8vo.—*From the Institute.*

Madras Journal of Literature and Science. New series. Vol. IV., No. 8. 8vo.—*From the Madras Literary Society.*

Monthly Return of Births, Deaths, and Marriages. Dec. 1859.—*From the Registrar-General.*

Beretning om en Zoologisk Reise i Sommeren, 1858, af D. C. Danielssen. 8vo. *From the Author.*

Diplomatarium Norvegicum. 1858. 8vo.—*From the Royal University of Christiania.*

Personalier oplœste ved Hans Majestaet Kong Oscar den I's. begravelse i Ridderholmskirken. 1859. 8vo.—*From the same.*

Tale Ved det Norske Universitets Mindefest for Kong Oscar, den 22de Sept. 1859, af J. S. Welhaven. 8vo.—*From the same.*

Al-Mufassal, opus de re grammatica Arabicum Auctore Abu'l-Kasim Mahmûd Bin Omar Zamahsario. Edidit J. P. Broch, Christiania. 1859. 8vo.—*From the same.*

Navne-Register. 1858. 8vo.—*From the same.*

Det Kongelige Norske Frederiks Universitets Aarsberetninger for 1856–58.—*From the same.*

Nyt Magazin for Naturvidenskaberne. 1859. 8vo. 10de Binds, 4de Hefte, and 11te Binds, 1ste Hefte.—*From the same.*

Karlamagnus Saga Ok Kappa Hans. Udgivet af C. R. Unger, Christiania. 1859. 8vo.—*From the same.*

Forhandlinger i Videnskabs-Selskabet I. Christiania. Aar 1858. 8vo.—*From the same.*

Uber die Geometrische Repräsentation der Gleichungen zwischen zwei veränderlichen, reellen oder komplexen Grössen, von C. A. Bjerknes. 1859. 4to.—*From the same.*

Proceedings of the Horticultural Society of London, No. 8. Jan. 1860. 8vo.—*From the Society.*